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Knowledge and Action.

JUST sixty years ago, Prof. T. H. Huxley, in a lecture on technical education, delivered to aid the Working Man's Club and Institute Union, used the significant phrase, "The great end of life is not knowledge but action". He went on to say that most people only need such knowledge as can be assimilated and organised into a basis for action, and that undigested learning is not only of no service to the individual or society, but also may be injurious. It was in the same lecture that Huxley, referring to the cost of education, made the oft-quoted statement, "I weigh my words when I say that if the nation could purchase a potential Watt, or Davy, or Faraday, at the cost of a hundred thousand pounds down, he would be dirt cheap at the money. It is a mere commonplace and everyday piece of knowledge, that what these three men did has produced untold millions of wealth, in the narrowest economical sense of the word."

Huxley's aphorism, and his application of it to profitable industry, provide an appropriate text for a short address to business men not alone in Nottingham but in any part of the world. In this country, however, we should be particularly proud of the international effects of the work of our own scientific and inventive citizens. We cannot too often remind the world that it was James Watt who transformed an imperfect pumping engine into an efficient and powerful working instrument for the use of steam power in industry everywhere; and he accomplished this by the combination of genius with scientific study and mechanical ingenuity. Every locomotive in the world originated in the applications of steam engines to railways by Trevithick and George Stephenson; and most of the steam turbines in use for marine propulsion, in power stations, and for various industrial purposes, are of the type designed and developed by Sir Charles Parsons, who brought about as great a revolution in the generation of power both ashore and afloat, as did Watt about a century earlier. When, in addition to the steam engine, the steam locomotive, and the steam turbine, in use in every civilised country, it is realised also that every electric dynamo owes its existence to Michael Faraday's discovery of the means of creating electric currents by the expenditure of mechanical energy, we may be pardoned for our pride in these contributions of our own countrymen to progressive industry all over the world.

Most of the inventions associated with the industrial revolution which began in the latter part of the eighteenth century were

products of the workshop and mechanical ingenuity and depended little upon the discovery of scientific principles in the laboratory. Since then, however, creative science has not only given rise to many practical applications and new inventions, but has added greatly to human comfort and welfare. From Faraday's discoveries at the Royal Institution, London, has developed the vast engineering industry, which in the light, power, and traction companies, electrochemical plant and the companies manufacturing electrical equipment and apparatus, provides employment for millions of workers. The radio industry is similarly the outcome of the scientific researches of Maxwell and Hertz on the properties of electric waves; and to Marconi belongs the credit of developing laboratory experiments into a system of communication through the ether. He was an engineer with vision, who put knowledge into action, and on that account his name is rightly associated with the commerical development of wireless telegraphy, and not because of his scientific contributions to the subject. As he himself acknowledged at the beginning of this century, "The experimental proof of Hertz, thirteen years ago, of the identity of light and electricity, and the knowledge of how to produce, and how to detect, these ether-waves, the existence of which had been so far unknown, made possible wireless telegraphy."

To foresee the possibilities of a discovery, to transform a laboratory experiment into the mechanical plant of a large works, or to apply it to the needs of ordinary life, requires aptitudes not commonly possessed by the scientific investigator. The engineer usually has such practical purposes in mind: discoveries are to him things to be used and not ends in themselves, as they are to the man of science. The two types of mind are, however, complementary to each other, and both are essential if knowledge is to be linked to action. Light itself is not made manifest until it comes into contact with matter; similarly, scientific knowledge has to be reflected in social service—intellectual or material—before its existence is recognised.

It may be said, therefore, that knowledge, like energy, is the power of doing work, and it only becomes effective when it is released. On the mechanical side, the potentialities of science are continually being made kinetic in inventions which add to human comfort and power; and these applications have revolutionised social and economic conditions. Industrial changes have been so great and rapid that it is no wonder that social instability exists everywhere. Our national leaders and administrators need wide knowledge and keen foresight to enable them to make the most effective use of the technical forces which are shaping the conditions of modern life. The scientific worker and technologist usually have their attention concentrated so closely upon their own particular subjects that they are indifferent to the social effects of their own activities.

Lack of familiarity with scientific matters has been an important factor in the crumbling away of our social structure under the stress of economic disaster, but we can stabilise the new social pattern by securing that action is related to knowledge. We are living in a period of great industrial and other material changes, the consequences of which can scarcely yet be predicted by the technical man, let alone be visualised by the average administrator. Equally, we are at the beginning of a new era of biological knowledge with new possibilities of physical and mental health, if our resources are planned intelligently with the object of satisfying real human needs.

It scarcely needs scientific authority to establish the principle that right nutrition, especially in early life, profoundly affects the well-being and social value of every individual member of a community. Much attention has been given in recent years to the determination of the essential constituents of a satisfactory diet, and the ascertainment of the financial resources of the general human population of this country to purchase such foods. Economists showed long ago, by records of family expenditure, that increases in income are associated with an increasing amount spent on food. What has been revealed by investigations carried out in recent years is that rising income, associated with increased consumption of milk, eggs, fruit, and a few other food-stuffs, goes hand in hand with decreased death-rate, better growth of children, greater adult stature, and much improved general health. On the other hand, it has been established that physique becomes worse, and disease more prevalent, as family income falls.

The results of investigations carried out by Sir John Orr and his staff at the Rowett Institute, Aberdeen, indicate that as many

as twenty million, or nearly one-half, of the population of Great Britain would have their health improved by an increased consumption of the more expensive food-stuffs such as fruit, vegetables, milk and animal products, while examinations of family food budgets and family incomes show that price is the limiting factor for the consumption of these things. In a very large proportion of the population, the income per head is less than that considered sufficient by scientific inquiry for the maintenance of optimum health. If consumption were increased to a level at which every member of the community had a diet adequate for health, there would be room for a considerable expansion of agriculture in this country even without decreasing imports.

The unfulfilled needs of the people of Great Britain for essential food-stuffs are assessed at £ 100,000,000 a year as a minimum, and at double that value to maintain a standard of perfect nutrition.

Scientific studies of nutrition, with their social implications, and in relation to agriculture, have raised world-wide problems. A year ago an International Committee of Physiologists appointed by the League of Nations reported on the kind of diet which would be fully adequate for health. The standard given by this Committee is at present reached only by the wealthier half of the population of Great Britain and by an even smaller proportion in many other countries. The final Report of a Committee of the League on the Relation of Nutrition to Health, Agriculture, and Economic Policy, published a few days ago, is the most authoritative and suggestive survey of the subject that has yet appeared and provides a convincing case for relating knowledge to action. "Millions of people in all parts of the globe", the Report states, "are either suffering from inadequate physical development or from disease due to malnutrition, or are living in a state of subnormal health which could be improved if they consumed more or different food. That this situation can exist in a world in which agricultural resources are so abundant and the arts of agriculture have been so improved that supply frequently tends to outstrip effective demand remains an outstanding challenge to constructive statesmanship and international co-operation."

These conclusions are obviously closely related to the national campaign to raise the general standard of physical fitness, and especially that of the young. Fitness can never be obtained by physical measures alone, be they educational or recreative: adequate nourishment is an essential factor in its attainment. Malnutrition is the root cause of poor physique, and increased attention to sports or physical training can never mitigate the ill-effects of a bad diet.

The measures recently instituted for improving the physical health of the people of Great Britain involve, therefore, the element of nutrition. The question of the physical condition of the community cannot be studied or handled as a whole on scientific lines unless all the relevant factors are taken into consideration at the same time. It is desirable, therefore, that national provision for physical training should be co-ordinated with measures for improving national health in a scheme conceived on wide and generous lines. No citizen or young person should be prevented by inadequate nourishment due to lack of means from benefiting by the opportunities provided to improve national physique.

National fitness thus involves not only provision for industrial progress, but also for the maintenance of healthy growth in the life of the individual and of the community. The degrading social consequences of the industrial revolution, and of the attitude of labour to mechanical inventions, were due to the neglect of these human factors. Both rightly or wrongly, science has been blamed for much of the wastage of life which has been brought about by the rapid applications of scientific knowledge to purposes of peace and of war. Men of science are, however, citizens as well as scientific workers; and they are beginning to realise their special responsibilities for securing that the fruits of scientific knowledge are used for human welfare. They can no longer remain indifferent to the social consequences of discovery and invention, or be silent while they are blamed for increasing powers of production of food supplies, providing means of superseding manual labour by machines and discovering substances which can be used for destructive purposes. It would be a betrayal of the scientific movement if scientific workers failed to play an active part in solving the social problems which their contributions to natural knowledge have created.

The view that the sole function of science

is the discovery and study of natural facts and principles without regard to the social implications of the knowledge gained, can no longer be maintained. It is being widely realised that science cannot be divorced from ethics or rightly absolve itself from the human responsibilities in the application of its discoveries to destructive purposes in war or economic disturbances in times of peace. Men of science can no longer stand aside from the social and political questions involved in the structure which has been built up from the materials provided by them, and which their discoveries may be used to destroy. It is their duty to assist in the establishment of a rational and harmonious social order out of the welter of human conflict into which the world has been thrown through the release of uncontrolled sources of industrial production and of lethal weapons.

In the consideration of the social aspects of science, scientific workers must be capable of appreciating the non-technical problems involved, as well as the scientific and technical issues, if they are to make an effective contribution to the scientific control of civilisation. They must, first of all, promote the extension of the application of scientific method to the consideration of social, economic and political questions, so that accurate knowledge may be obtained upon which sound conclusions may be based and progressive policies or programmes established. The task of securing action upon the facts is scarcely one for scientific workers as such; though it is their responsibility as individual private citizens to do all in their power to secure the appropriate action. They have, however, a further public duty which they cannot lightly evade; and that is the task of awakening public opinion to the grave danger incurred in the neglect to take action along the lines indicated by the results of impartial and scientific inquiry. The British Association is assisting to this end at its meetings by the discussions and papers bearing upon the relation between the advance of science and the life of the community.

RICHARD A. GREGORY.

Address given at a luncheon of Nottingham City Business Club on Friday, September 3rd, 1937, in connection with the Annual Meeting of the British Association.

Henry Edward Armstrong.

H. E. ARMSTRONG, a striking personality in chemistry, striking alike for independent-mindedness as for the relentless fervour with which he expounded the results of his labours, passed away on July 13, at the age of ninety (*b.* May 6, 1848). His was a vigorous and active life, and even a week before his death, he is reported to have corrected the proof of a scientific contribution "Ammonolatry—The Life Element", appearing in *Nature* (July 24, 1937), representing "the final expression of the frank and critical views which he held upon the training of chemists and subjects of research". As a critic he was unsparing and provocative, and this he perhaps owed to his illustrious professor, Kolbe. Voluminous and versatile were his contributions to chemistry, which included the studies on structure of naphthalenes, structure of camphor and mechanism of chemical changes. His work on the nature and mode of action of enzymes, in which among others his son E. F. Armstrong collaborated, is to be found recorded in the 23 papers appearing in the *Proceedings of the Royal Society*. He was associated with numerous educational and agricultural bodies. His favourite hobby was field geology and he was fond of open country and frequent travel. Among his associates, mention may be made of Kipping, Lowry and Forster. He was essentially an individualist and was "somewhat arrogant and extreme perhaps in his denunciations", but was a kind hearted gentleman whose "devotion to chemistry was the master passion of his life". In this connection, *Chemistry and Industry* (July 17, 1937) observes "We venture to think that for a long time, whenever old or middle-aged chemists meet they will think of him with affectionate remembrance and regret that there is no one left who can do so much to give life and vigour to chemical discussion, instruct and to divert chemists and enliven their minds.... Other men will make notable advances in the science of chemistry and invent processes of value to industry; others will advocate the proper place for chemistry in our national life. We doubt whether a chemist with so sure a judgement on many chemical matters will again invent a new waist coat of many colours, invent a familiar nick-name for a scientific journal and do so much to brighten our chemical outlook."

The Definition of Physical Quantities.

By H. J. Taylor, M.Sc., Ph.D., *Wilson College, Bombay.*

IN the July issue of *Current Science* appears a review of a recent text-book of mine. On the whole it is a careful and generous review, and I have no wish to controvert it in detail, although with regard to some of the criticisms there may well be a legitimate difference of opinion. The reviewer, however, raises points of a fundamental character, intrinsically more important than the merits or demerits of my book, deserving a full and critical discussion. These refer to the fundamental physical quantities, and the method by which they are to be approached and defined. The modern point of view on this question differs radically from that of the older generation of physicists, which was common, let us say, half a century ago.

On the older view, we approach the fundamental quantities from the standpoint of our intuitive knowledge. We have certain conceptions which belong to the familiar world of everyday experience. How these conceptions are acquired is a matter worthy of careful examination, but this point we will for the moment avoid, merely noting that they are there. Thus we recognize a certain 'massiveness' or 'substantiality' as a characteristic of matter, which is the lay equivalent of the scientific concept of mass. Again, the familiar notions of hotness and coldness correspond to the scientific concept of temperature. So too with length and time, which are recognised and known intuitively, quite apart from scientific measurement.

On this view, the process of scientific definition is simply one of the progressive refinement of conceptions. We envisage the quantities as already existing and already known, though perhaps in a rough inexact way. This increasing refinement may be illustrated by the progress of thermometry. From general ideas of hotness and coldness we proceed to Newton's scale of 'degrees of heat' (*e.g.*, "12 degrees: about the heat of a bird hatching its eggs....192 degrees: the heat of coals in a little kitchen fire...."[1] The next step is to prescribe the construction of a mercury thermometer, which enables us to 'measure' the temperature of bodies. Afterwards we have the gas thermometer, then the perfect gas scale, and finally the thermodynamic scale of temperature. It is maintained that we have here methods of measuring temperature which give us, with increasing precision, the value of the 'true temperature' of a body. The 'true temperature' is conceived as a property of the body, having a perfectly definite value, which exists quite apart from our attempts to measure it.

Length, mass and time are conceived in much the same way as intrinsic entities, to which the process of measurement is applied, so to speak, from outside. It is admitted that the exact content of these conceptions cannot be stated fully in words, and the student is therefore referred to his own experience. It is assumed that everyone knows what length is; all the physicist has to do is to discover some accurate method of measuring it.

This view of physical quantities was almost universal until about half a century ago. In such an excellent work as Ganot's *Physics*, for example, (then widely used), this view is implicit in the whole treatment. At the present day, it is still the common method of approach in elementary works, and the quantities are introduced to the student by some general discussion of their everyday counterparts. To take an illustration almost at random, the following way of introducing the idea of temperature is adopted in Kilby's *Introduction to College Physics*: "Ice is said to be cold and steam is said to be hot, but cold and hot are only relative terms. One body is relatively hotter than another when heat will flow from it to the other body, provided they are in thermal communications. However, the quantity of heat that flows, or that is in a body, does not determine its *temperature*, which is defined as a body's thermal condition—its hotness or coldness. But in order to specify the temperature of a body some standard thermal condition, such as that of melting ice, must be used for comparison." It is surely obvious that this definition begs the whole question. Most authors are rather uncomfortable about these introductory remarks, and hurry past them as rapidly as possible.

Even half a century ago quantities (such as *action* and *entropy*) had appeared in physics, to which it was difficult to assign any familiar counterpart. The exact status of these quantities was the subject of much discussion. It was generally held, I think, that entropy was a useful and very

[1] *Phil. Trans.*, Abridged, **2**, 1701.

convenient integral occurring in thermodynamical theory, but not a physical quantity having the same sort of objective reality as temperature or heat.

The fallacies underlying this intuitive approach to physics were apparent even before the days of relativity to such acute thinkers as Poincaré (*Science and Hypothesis* see especially Chap. VI). The new views became more widely known when the theory of relativity had made it clear beyond all doubt that the only quantities which can be built into a consistent physical scheme are the quantities which result from actual measurement; not the quantities of which we think we have intuitive knowledge. The Newtonian scheme of physics is based on our intuitive conceptions of space and time. In this scheme there is a unique time-relation between any given pair of events, corresponding to our conception of a definite interval elapsing between their moments of occurrence. This interval is conceived to exist quite independently of our observing it, and it is presumed (since physical time is taken to be only a refined version of intuitive time) that measured time intervals will exhibit this same character of uniqueness. We find in practice, however, that the measured time-relation between two events does not actually have this character. If therefore we insist on ascribing uniqueness to it we are led to a theory which is contradicted by the observations.

In modern physical theory length does not appear as an inherent gulf fixed in Nature between two points. It appears as the result of a set of operations with metre rods, performed in a prescribed way. It is precisely this numerical result which is carried into the theory under the name 'length'. It must be emphasized that the result of the prescribed operations *is* the physical quantity 'length', and similar remarks apply to all our other physical quantities. Physical quantities are thus in a sense manufactured articles. As Eddington remarks: "We shall not define a physical quantity as though it were a feature in the world-picture which had to be sought out. *A physical quantity is defined by the series of operations and calculations of which it is the result.*" (*Mathematical Theory of Relativity*; Introduction.)

To define a physical quantity, then, we must prescribe a set of operations. For example, to define length it is first agreed that a particular piece of metal in Paris shall be called a standard metre, and we manufacture copies of it as exactly as possible. We then state a procedure for determining a length AB, namely that standard metres are to be laid down in a straight line between A and B, and the number which can be thus placed is to be counted. This number *is* the length AB. (Fractional values are taken into account by subdividing the unit into standard centimetres, etc.) Experiment shows that the length so determined does not have a unique value, but depends on the motion of the observer. Classical physics endeavours to save the situation by means of an *ad hoc* hypothesis (the Fitzgerald contraction); modern physics simply accepts the facts and recognizes that the quantity defined as length is not an absolute relation between A and B.

In practice more roundabout methods are used for determining length, but they are only valid to the extent to which they are equivalent to the above procedure. A surveyor works from a base line laid out according to the definition, his subsequent measurements are made with a theodolite, utilizing the properties of rays of light. If it turned out that lengths measured in this way disagreed with direct metre-rod determinations it would be necessary to decide which method was legitimate, *i.e.*, to re-define length. In the measurement of mass it is generally recognized that there actually are two alternative procedures, and we distinguish accordingly between gravitational and inertial mass, even though experiment shows that the two are numerically identical. As another illustration may be mentioned the discrepancy in the determinations of e/m for electrons, corresponding to the two types of measurement, crystal diffraction and direct deflection. If this discrepancy proves to be genuine we shall have to distinguish two e/m's, corresponding to the two methods of determination.

Returning to temperature, we now see that it can *only* be defined as the reading on a standard thermometer, *i.e.*, by referring it to the system of operations by which it is obtained. Writers on heat admit this implicitly, since, after the introductory chat about hotness, they devote a chapter to the proper method of constructing a thermometer. This chapter forms the real definition, and we hear no more about hotness. (This concerns, of course, one's first approach to the concept of temperature. At a later stage in the theory of heat, temperature is related to length, mass, and time, and may then be re-defined by reference to these. But in the earlier stages it is on the same

footing as the three fundamental quantities, and must be defined accordingly.) To show that temperature has no meaning apart from a thermometer, we have only to ask the question "What is the temperature of interstellar space?" The question as it stands is meaningless, and only acquires a meaning when we have prescribed a procedure by which we imagine the temperature to be determined. Judged by the energy of the residual molecules this temperature may be very high: judged by the intensity of radiation it is certainly very low. In this connection the reviewer wonders why I do not "define force as the indication of a standard dynamometer". This is precisely how force *is* defined, according to Newton's second law. The ideal dynamometer is the free massive particle and the 'indication' is its measured acceleration. In practice, we may use other types of dynamometer but this is merely a matter of practical convenience.

In the quantum theory it is made even clearer that physical quantities are the results of certain operations of measurement and calculation. Unobservables are excluded from the scheme. We have an intuitive conception of a particle in definite place having a definite velocity but we cannot locate electrons by extending this conception to the atom. If we insist on treating electrons along the lines of our intuitive conceptions as miniature billiard balls we are led to a scheme (the earlier Bohr theory) which is contradicted by the more refined observations.

Now it seems to me that in the teaching of physics our definitions, indeed our whole approach to the subject, must conform to what we now know of the nature of physical quantities. Such a definition as "mass is the quantity of matter in a body" is objectionable on two grounds: (*a*) It is manifestly wrong, and has been known to be wrong since the mass of radiation was first recognized. The only possible way of attaching a meaning to the expression "quantity of matter" is to refer it to the number of protons and electrons in a body, and this number is certainly not equivalent to mass in any sense. (*b*) It represents an appeal to intuitive conceptions, which, as indicated above, is a false appeal. I define mass unambiguously from the practical standpoint as the result of certain operations involving a balance and a standard kilogram. Poincaré was even more radical; after a brilliant analysis of the various ideas involved he concluded that the only possible definition was "Masses are coefficients which it is found convenient to introduce into calculations".

Let me not be misunderstood. I am not advocating the introduction of advanced physics into elementary teaching. I am in agreement with Planck on this point, who remarks, "I should consider it extremely dangerous if the intermediate schools were to deal with the theory of relativity or the quantum theory....and I would definitely condemn any attempt to take such a question as that of the universal validity of the principle of conservation of energy—which, of course, to-day is seriously regarded as an open one in nuclear physics—and treat it as debatable before pupils who cannot have properly grasped the meaning of the principle involved, much less its potential scope."* No, the solution of our difficulties does not lie in anticipating advanced work in the elementary stages, but surely the wider view gained in these higher flights may help to determine the direction in which we shall walk on the more pedestrian levels.

The biologists tell us that an organism, in its embryonic stages, recapitulates the past history of its race. I cannot see why this should also be regarded as necessary in educational method. Why should a student be required to recapitulate the mistakes of his predecessors? Why should he not start at once on the right lines? The modren ideas are not intrinsically more difficult than any others, but only less familiar. I find in practice that students grasp these modern definitions quite readily. This is to be expected, since what is clear and explicit is more acceptable to the mind than what is confused and hazy. For beginners the presentation must naturally be made very simple (as in every other kind of teaching). This simplification, I find, offers no difficulty, and even the youngest students can grasp the main essentials.

"What is learnt", says Planck, "is not as important as how it is learnt". But in the teaching of physics there is, most unfortunately, a tremendous weight of the most deadly formality and conservatism, which makes any departure from tradition a most difficult task. The nature of physical quantities is surely a fundamental question, and my remarks will have fulfilled a useful purpose if they serve to stimulate a more general interest in such questions.

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* Planck, *The Philosophy of Physics* (Geo. Allen & Unwin, 1936).

Anti-jassid Resistance in the Cotton Plant.

By K. B. LAL, M.Sc., Ph.D. (Edin.), F.R.E.S.,
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THE mechanism by which certain varieties of cotton resist jassid attack is still largely obscure. In addition to actual resistance there seems to be some, what may be called, jassid avoidance or jassid endurance in the plant brought about by a change in environment or cultural practices. It has been stated that the resistance of a variety to jassid attack is the result of (*i*) its better adaptability to unfavourable climatic conditions, specially drought (Hargreaves, 1934),[1] (*ii*) an abundance of rich fertilisers, specially potash, in the soil (Vuillet, 1925),[2] (*iii*) freedom of the soil from a water-logged condition (Parnell),[3] etc. Some or all of such conditions may possibly at times be helpful to the crop and confer on any particular variety the semblance of jassid resistance.

Real resistance, however, may only result from a character or characters of the plant itself that will withstand jassid attack more or less independently of cultural factors. Of such the only one suggested, so far, is the hairiness of cotton leaves. Cotton varieties with hairy leaves usually resist jassid attack: this is no doubt true to a certain extent, but cases in which this rule breaks down point to the possibility of this association being merely of an incidental nature. The Punjab-American cotton 43F is one of the most hairy of varieties; yet its resistance against jassids, though higher than those of some very susceptible varieties such as 38F, 45F, and 289F is not as high as that of 4F for instance. Indeed at times it may be heavily attacked as was borne out by an observation, last August, of some plots under this variety in Sargodha District, Punjab. Similarly, Jublee cotton (a *desi* variety newly evolved at Lyallpur) is a very hairy variety even among *desi* cottons but its resistance to jassid attack, though better than those of the American varieties, is about the lowest among *desis*.

Although hairiness of cotton leaves has been prominently mentionad as a jassid resistant character, published records giving the comparative hairiness of different varieties of cotton, based on a satisfactory technique, are not many. Since hairs on cotton leaves are deciduous (Afzal, 1936),[4] hairiness will vary not only with variety, but also with the age of the leaves, their position on the stem and the age of the plants. Hairiness of different varieties, therefore, is comparable only when due regard has been paid to these three factors in selecting leaves for examination. The table of hairiness of cotton varieties recently given by Luthra (1937)[5] suffers from this defect.

Hairiness of leaves usually refers to their hair density, though from the standpoint of the infesting insect, hair length cannot altogether be ignored, specially as adjoining hairs mat together and form a felt-like covering on the leaf surface and their length coupled with their density determines the thickness of this covering. The writer measured the comparative hairiness on the lower surface of leaves, both in respect of density and length, of seven varieties of cotton in August 1936, at Lyallpur. Five leaves from each variety, all of the same age and plucked from identical positions on the main stems of plants also of the same age, were selected. Each leaf was examined at five places corresponding to its five lobes, for hairiness and for hair lengths, and the following table gives the hairiness and length of hair of the leaves of each variety as averages of all these figures for each variety :—*

Variety	No. of hairs per sq. cm. of leaf area	Average length of hair in mm.
1. 38F	556	0·826
2. 45F	372	0·834
3. 43F	1306	0·901
4. 47F	1215	0·625
5. 289F	591	0·807
6. Labh Singh Selection ..	1152	0·665
7. Jublee cotton	1626	0·486

Here the resistant varieties have short hairs and high hair density, though the most hairy American variety which also has the longest hair length is not the most resistant. The table also indicates the need of testing the rôle of hairiness in resisting jassid attack

* Fuller details are being published elsewhere.

by evidence other than merely that of correlation.

Parnell (*op. cit.*)[3] observed that jassid nymphs, when put on hairy and much less hairy cotton leaves, always showed their dislike of the former by kicking and knocking about, although apparently in great need of food, while those on the latter quietly settled down to feed.

Similar experiments were carried out by the writer in which nymphs of *Empoasca devastans* Distant of II, III and IV instars were starved for periods varying from 4 to 6½ hours and put on fresh leaves of *Rosæ* 10, *Indicum* 50, *Mollisoni* 15, *Mollisoni* 39, Jublee cotton, 47F and 45F. In no case were the nymphs observed kicking and knocking about as observed by Parnell, although all the varieties except 45F are very hairy. The nymphs always took to the variety quietly and started feeding. These observations were continued constantly for 15 minutes after which they were allowed to remain on them for another 24 hours. Practically all of them easily survived this period and moulted in the normal manner. It is, therefore, difficult to confirm Parnell's findings in this respect. Incidentally the experiment also shows that the feeding ability of the nymphs has not much to do with producing immunity in varieties of cotton.

There is some indication that cotton varieties resist jassid attack because of the inability of the latter to oviposit on them.† If this be so, it shows that jassid-resistant characters must be sought for in the leaf veins since it is in these that eggs are laid. A knowledge of the physical and chemical characters of the leaf veins, therefore, might furnish a clue to the very small numbers of eggs laid in the leaf veins of resistant varieties. It was in view of this fact that in every examination of leaf areas for the determination of their hairiness (*vide* table given above) a part of one of the principal leaf veins was invariably included. Veins, however, do not seem to be appreciably more or less hairy than the general surface of the leaf which they serve. The determination of the relative toughness of the leaf veins in different cotton varieties appears to be a more promising line of investigation from the standpoint of their resistance to jassid attack.

It is, however, doubtful if any character or characters would safeguard cotton varieties against jassids under all circumstances and there seems some truth in the statement of Bebbington and Allen (1937)[6] that 'jassids may cause quite serious damage even to the most resistant strains if they are improperly grown'. The South African cotton U4, though resistant to jassid attack in its native habitat, becomes susceptible when planted in the Punjab. In this case one of the two things might happen: either the U4 cotton imperceptibly sheds its jassid-resistant character under Punjab conditions or the character or characters that protected it against *Empoasca facialis* Jac., the South African species, do not afford it the same protection against *E. devastans* Distant, the Indian species. There appears to be sufficient justification for studying the question of jassid-resistance primarily as local problems, in relation to the species of the insect and a limited number of cotton varieties concerned.

† Evidence on this point is being published elsewhere.

[1] Hargreaves, H., *Rept. Proc. II. Conf. Cott. Grg. Prob.*, 1934.

[2] Vuillet, J., *Rev. App. Ent.*, (A), 1925, pp. 74–75.

[3] Parnell, F. R., *Rept. Proc. II. Conf. Cott. Grg. Prob.*, 1934.

[4] Afzal, M., *Ind. Jour. Agric. Sci.*, 1936, p. 824.

[5] Luthra, R. S. J. C., *Curr. Sci.*, 1937, p. 596.

[6] Bebbington, A. G., and Allen, W., *Emp. Cott. Grg. Rev.*, 1937, p. 31.

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The Reciprocity Theorem in Colloid Optics.

IN an earlier paper[1] I had derived the following algebraic relation

$$\rho_u = (1 + 1/\rho_h)/(1 + 1/\rho_v) \quad .. \quad (1)$$

between the depolarisations ρ_u, ρ_v and ρ_h of the Tyndall scattering in colloids, where ρ_u, ρ_v and ρ_h are the measures of the depolarisation of the transversely scattered light when the incident light is respectively (1) unpolarised, (2) polarised with its electric vector vertical and (3) polarised with its electric vector horizontal, the plane of observation being horizontal. The validity of the above relation for various colloidal systems, emulsions, suspensions, protein solutions, etc., irrespective of size, shape, structure and distribution of particles contained in them has been verified experimentally by myself[2] and Subbaramiah.[3] Recently, Gehman and Field[4] have shown that in rubber solutions also the observed values of ρ_u, ρ_v and ρ_h satisfy the above relation.

In a recent paper in the *Physikalische Zeitschrift* Prof. Gans[5] has indicated that the relation (1) is not correct and has derived an alternative relation connecting the three quantities ρ_u, ρ_v and ρ_h given by

$$1/\rho_u = \tfrac{1}{2}\,(1/\rho_v + \rho_h)^* \quad .. \quad .. \quad (2)$$

However, he finds that the values of ρ_u, ρ_v and ρ_h obtained by Subbaramiah and myself satisfy the relation (1) and not the relation (2). He explains this discrepancy as due to the possible errors in the measurement of the small depolarisation factor ρ_v. It can be shown that the possible errors in ρ_v cannot, however, account for the large deviations from his formula. Besides, in the case of colloidal solutions containing highly anisotropic particles, *i.e.*, possessing comparatively high value of ρ_v the errors in the measurement of ρ_v are reduced to a great extent. It is found[6] that even in such cases the relation (1) is valid and not relation (2).

As has already been pointed out by me,[7] the relation (1) admits of a more simple and direct experimental test in which the various sources of error possible in the determination of the depolarisation and consequently in testing equation (1)

* Gans has employed the following notations Δ_u, Δ_v and Δ_h corresponding to ρ_u, ρ_v and $1/\rho_h$ used by me.

are avoided. The principle of this method consists in splitting the incident unpolarised light into two beams of equal intensity, one with vibrations vertical and the other with vibrations horizontal, by passing through a double-image prism. The colloidal solution is placed in the track of both the beams which enter it side by side and the light scattered transversely is viewed through a second double-image prism. Four images of the track would then be seen corresponding respectively to the components V_v, H_v, V_h and H_h (the first two arising from the vertical and V_v and H_h have finite values. The photograph taken with castor oil emulsion and reproduced in Fig. 3 illustrates this fact very clearly. It is well known that emulsions usually contain droplets of spherical shape and consequently the middle two anisotropic components H_v and V_h should be absent. This is actually the case (see Fig. 3). This further illustrates that Gans' relation does not stand the test of experiment.

The arguments of Gans are apparently defective in that no account has been taken of the magnetic partial waves as also the

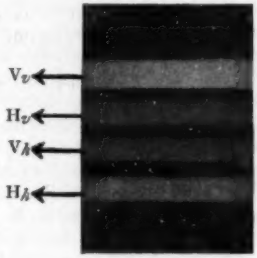

Fig. 1.
Silver sol.

Fig. 2.
Arsenic trisulphide sol.

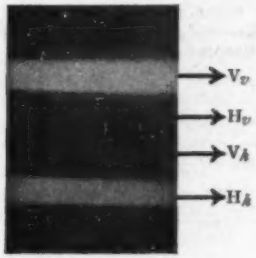

Fig. 3.
Castor oil emulsion.

component and the last two from the horizontal component of the incident beam). Typical photographs are reproduced in Figs. 1–3.

According to Gans the relative intensities of the four components should be as follows : $V_v > H_v = H_h > V_h$, whereas according to me $V_v > H_h > H_v = V_h$. In the various cases studied, it is found that the two middle components, *i.e.*, H_v and V_h, are always exactly identical in intensity and colour, establishing thereby the validity of the relation given by me and not that given by Gans. The colour and intensity of the two outermost components differ greatly from each other and from those of the middle components, and these differences furnish indications regarding the size and shape of the scattering particles (see Figs. 1–3).

Further, according to Gans, for colloidal spherical particles whose size is not small compared with the wave-length of light $H_v = H_h = 0$ and consequently ρ_u is equal to zero whatever may the size of the scattering particles. This is in contradiction to Mie's theory[8] according to which $H_v = V_h = 0$ higher electric partial waves as indicated in Mie's theory.

A more detailed paper will be published elsewhere.

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September 12, 1937.

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[1] Krishnan, R. S., *Proc. Ind. Acad. Sci.*, (A), 1935, **1**, 782.

[2] ————, *ibid.*, 1935, **1**, 915; 1935, **2**, 221; 1936, **3**, 211; 1937, **5**, 94, 305, 407, 499, 551 and 577.

[3] Subbaramiah, K., *ibid.*, 1937, **5**, 128 and 138.

[4] Gehman, S. D., and Field, J. E., *Industrial and Engineering Chemistry*, 1937, **29**, 793.

[5] Gans, R., *Phys. Zeits.*, 1937, **38**, 625.

[6] Krishnan, R. S., *Proc. Ind. Acad. Sci.*, (A), 1937, **5**, 499.

[7] ————, *ibid.*, 1935, **1**, 782.

[8] Mie, G., *Ann. d. Phys.*, 1908, **25**, 377.

The Intensity of Raman Lines due to Intermolecular Oscillations.

In an article on the Raman effect and molecular structure published recently in *Current Science*,[1] Venkateswaran has discussed the origin of the new lines which are observed in the neighbourhood of the Rayleigh line in the Raman spectra of some substances in the solid state. In a previous investigation the present writer observed that in the spectrum of light scattered by solid carbon disulphide there are two new Raman lines due to intermolecular vibrations, one of which is many times more intense than the line 656 cm.$^{-1}$ of carbon disulphide and it was suggested that the new lines are due to oscillations in polymerised groups of CS_2 molecules which are probably present in solid carbon disulphide. In a recent paper the present writer and Gupta[2] pointed out that since the strong new Raman line of CS_2 due to intermolecular oscillations is many times more intense than that due to the symmetric breathing oscillation of the CS_2 molecule, the origin of these new lines cannot be attributed to lattice oscillations, because a lattice oscillation cannot produce a change in the polarisability greater than that produced by the breathing oscillation of the molecule. Venkateswaran has pointed out in the discussion mentioned above that the new lines are due to lattice oscillations and that lattice oscillations can produce intense Raman lines because, according to him, the intense Raman line of diamond is due to such an oscillation. For two reasons, however, it is not relevant to cite the example of diamond in order to prove that a lattice oscillation can produce a Raman line more intense than that due to the breathing oscillation of the molecule. First, in the case of diamond there is only one strong Raman line unlike in the case of carbon disulphide in which there are two intense Raman lines, one due to the intermolecular vibration and the other due to the breathing oscillations of the molecule. Secondly, the vibration which is responsible for the origin of the strong Raman line of diamond is not a lattice oscillation in the sense in which the term is applied here. Any mode of oscillation is, no doubt, a lattice oscillation; but of these oscillations, those in which the chemical bonds among the adjacent atoms undergo contraction or bending are called "inner vibration" of the molecules or groups of atoms in the solid state and the remaining vibrations in which no such change takes place in the bonds are called lattice oscillations. In the case of a non-ionic or molecular lattice, the former may also be called *intramolecular* and the latter *intermolecular* oscillations. It is well known that in the diamond lattice each carbon atom is linked chemically to four adjacent carbon atoms by C—C bonds and therefore the whole crystal behaves as a single molecule as far as these oscillations are concerned. The oscillation of the diamond lattice produces the above-mentioned changes in these C—C bonds resulting in large changes in the polarisability and consequently the corresponding Raman line is very intense. Such an oscillation, however, is quite different from the intermolecular oscillation in solid carbon disulphide if there be no chemical linkage among different CS_2 molecules. It is not possible to imagine different groups of carbon atoms in the diamond lattice among which there are no chemical linkages. Hence it is evident that the case of diamond is not a suitable example to prove that lattice oscillation can produce Raman lines more intense than the breathing oscillation of the single molecule.

As regards the statement made by Venkateswaran that there may occur more than one Raman line due to oscillations of a lattice other than cubic, it may be pointed out that the case of sulphur does not corroborate this statement, because according to his own statement[3] only one new Raman line appears in the solid state though the crystal belongs to the orthorhombic system.

Without discussing the other indirect evidences it may be mentioned here that in a paper communicated to the *Indian Journal of Physics* recently by the present writer and Gupta it has been shown that the lattice of *p*-dichlorobenzene is the same at 18° C. as at 39° C. though the frequencies of some of the new Raman lines close to the Rayleigh line are different at these two temperatures. It is evident, therefore, that these lines are not due to lattice oscillations.

As regards the results of investigation of the Raman spectra of mixed crystals reported by Vuks[4] and referred to by Venkateswaran, it may be mentioned that the present writer has repeated these investigations in collaboration with Bishui and has not been able to confirm the conclusions arrived at

by Vuks. These results will be published shortly in the *Indian Journal of Physics*.

The writer is indebted to Prof. D. M. Bose for kindly drawing his (writer's) attention to the article of Venkateswaran mentioned above.

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Palit Laboratory of Physics,
92, Upper Circular Road,
Calcutta,
August 3, 1937.

[1] Venkateswaran, C. S., *Curr. Sci.*, 1937, **6**, 11.
[2] Sirkar, S. C., and Gupta, J., *Ind. J. Phys.*, 1937, **11**, 55.
[3] Venkateswaran, C. S., *Proc. Ind. Acad. Sci.*, 1936, **4**, 414.
[4] Vuks, M., *Acta. Phys. Chim. U. S. S. R.*, 1937 **32**, 6.

IN the letter by Sirkar on the "Intensity of Raman lines due to intermolecular oscillations", he adduces evidence in support of his hypothesis that the low frequency Raman lines observed in light scattered by solids are not due to lattice vibration, but probably to intermolecular oscillations of polymerised groups of molecules in solid state. In my discussion on the problem referred to in his letter, diamond was pointed out by me as an example of a crystal lattice yielding intense Raman line, for the reason that, it is the one case of a solid in which it has been shown by rigorous mathematical analysis that the oscillation of one of the two interpenetrating cubes against the other gives rise to the depolarised line. This vibration of the diamond molecule is essentially different from the inner vibrations of the atoms within the molecule itself and is generally understood as a lattice oscillation, though, no doubt, as Sirkar has rightly pointed out, the forces connecting the units of the lattice are valence forces. In the case of the organic compounds the lattice units in the crystal are linked together by Van der Waal's forces, which are, of course, very much feebler than the valence bonds. These linkages would also involve changes in polarisability and yield the low frequency lines as much as the co-valent linkage in diamond, co-ordinate linkage in quartz and ionic linkage in calcite and aragonite as reported by earlier workers. On the other hand, if these lines were due to intermolecular oscillations of polymers, they should be more or less polarised; but the experimental facts show that most of the intense lines in this region are depolarised. Recently Magat[1] has pointed out this difficulty to reconcile with the assignment of the broad, intense band of water at about 176 cm.$^{-1}$ to the breathing frequency of the tetrahedral grouping of the polymerised water molecule. Besides, it is difficult to understand the mechanism of polymerisation in CS_2 and CCl_4 in which Sirkar has reported low frequency lines.

Regarding the number of these crystal lines, it is not justifiable to draw any conclusion from the fact that only one line has been reported by me for the ortho-rhombic crystals of sulphur. More lines falling in the region of the Rayleigh line would well have escaped detection for two reasons: (1) the dispersion of the spectrograph used in the region λ 5461 is only about 60 A.U. per mm. and (2) the spreading of the exciting line is great on long exposure which is necessary to obtain these lines due to the feeble scattering power of the green light.

Sirkar reports that the results with *p*-dichlorobenzene show that these lines are not due to lattice oscillations. It may be mentioned that exactly opposite results have been reported by Vuks[2] in a recent communication, which seem to support the lattice theory of these oscillations. The investigations of these two authors on different crystalline forms as also on mixed crystals are thus apparently contradictory. A full account of the experimental results of Sirkar is awaited with great interest.

C. S. VENKATESWARAN.

Indian Institute of Science,
Bangalore,
August 24, 1937.

[1] Magat, M., *Trans. Far. Soc.*, 1937, **33**, 114.
[2] Vuks, M., *Acta. Phys. Chim. U. S. S. R.*, 1937, **6**, 11.

Aluminium Chloride, a New Reagent for the Condensation of β-Ketonic Esters with Phenols.

THE condensation of β-ketonic esters with phenols has been extensively studied in recent years, the principal condensing agents used being concentrated sulphuric acid (Pechmann reaction[1]) and phosphoric anhydride (Simonis reaction[2]). The condensing agents used to a much smaller extent are

hydrogen chloride, phosphoryl chloride, zinc chloride in presence of hydrogen chloride, phosphoric acid, and sodium ethoxide.[3]

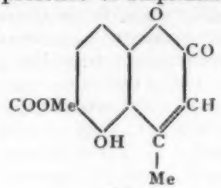
It has now been found that anhydrous aluminium chloride acts as an efficient condensing agent for the condensation of β-ketonic esters with phenols, the condensation being effected in ether solution[4] in which aluminium chloride readily dissolves, or in nitrobenzene solution.

Some of the interesting results which have been so far obtained are the following :—

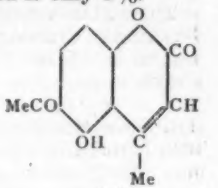
1. Methyl β-resorcylate on condensation with ethyl acetoacetate by the new method affords mainly methyl 5-hydroxy-4-methyl-coumarin-6-carboxylate (I), m.p. 185–186° in good yield, from which by hydrolysis and subsequent decorboxylation, 4-methyl-5-hydroxy coumarin, m.p. 263°, was obtained. Condensation with sulphuric acid gives methyl 4-methyl-7-hydroxy-coumarin-6-carboxylate,[5] m.p. 212–214°. The facile formation of 5-hydroxy-coumarin derivatives in this and subsequent cases by the new method is of unique interest, as 5-hydroxy-coumarin derivatives, which otherwise cannot be obtained or can be only obtained with great difficulty, can be readily prepared. Limaye[6] has recently reported the formation of 5-hydroxy-4-methyl-coumarin in a minute yield as a bye-product in the Kostanecki acetylation of 2-acetyl resorcinol.

2. Resacetophenone condenses with ethyl-acetoacetate similarly to methyl β-resorcylate, the product obtained in high yield being 4-methyl-5-hydroxy-6-acetyl–coumarin, m.p. 165° (II). The constitution of (II) has been established and confirmed by its synthesis by the Fries transformation of 4-methyl-5-acetoxy-coumarin. The observation of Aggarwal and Dutt[7] that 4-methyl-7-hydroxy-6-acetyl-coumarin, m.p. 147°, is formed with concentrated sulphuric acid or sodium ethoxide as condensing agent could not be confirmed, only unchanged resacetophenone, m.p. 144°, being obtained in both the cases.

3. The condensation of some simple phenols gives in most cases the same product as those with sulphuric acid. In many cases, the yields are better by the new method than those with sulphuric acid, the most remarkable example being the condensation of phenol and ethyl acetoacetate which gives a yield of 30–40% of 4-methyl-coumarin ; the reported yield of the coumarin in literature for the same condensation in presence of sulphuric acid is only 3%.



(I)



(II)

A detailed account of the above investigation will shortly be published elsewhere. Further work, including other condensations of phenols and substituted phenols with β-ketonic esters in general is under progress.

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Karnatak College, Dharwar.

Ismail College, Andheri, Bombay.

August 14, 1937.

[1] Pechmann and Duisberg, *Ber.*, 1883, **16**, 2119.
[2] Petschek and Simonis, *ibid.*, 1913, **46**, 2015.
[3] Naik, Desai and Desai, 1929, **6**, 801 ; Chakravarti, *J. Ind. Chem. Soc.*, 1935, **12**, 536 ; Appel, *J.*, 1935, 1031.
[4] Shah, *Curr. Sci.*, 1934, 157.
[5] Shah and Sethna, *Ind. Sci. Cong. Abst.*, 1937, 21.
[6] *Rasayanam*, 1936, 45.
[7] *J. Ind. Chem. Soc.*, 1937, **14**, 109.
[8] Clayton, *J.*, 1908, 2018.

A Direct Synthesis of Dihydro-*iso*lauronolic- and *iso*lauronolic Acids.

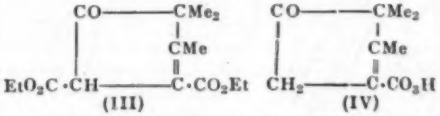
Ethyl *iso*lauronolate was first isolated by Walker[1] amongst the products of electrolysis of sodium salt of α-ethyl-hydrogen-camphorate. The free acid (V) has been prepared (*i*) by the action of aluminium chloride on camphoric anhydride,[2] (*ii*) by distillation of sulphocamphylic acid in superheated steam,[3] and (*iii*) by treating α-campholytic acid with dilute sulphuric acid. Its formation from the above camphor derivatives involves a peculiar molecular rearrangement.

Although Blanc[4] synthesised *iso*lauronolic acid starting from αα-dimethyladipic acid *via* *iso*laurolene and dihydro-*iso*lauronolic acid, involving the addition of a $COCH_3$ group which permits its occupying an alternative position, it was considered desirable to effect a direct synthesis of this acid.

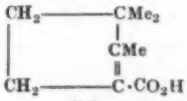
Ethyl dimethyl-acetocetate reacts with ethyl-α-bromosuccinate in presence of zinc to give a mixture of ethyl α : α : β-trimethyl-β-hydroxy-β′-carbethoxy adipate (I) (acid m.p. 165–66°) and ethyl α : α : β-trimethyl-γ-carbethoxy-β-butylene-α : δ-dicarboxylate (II). The mixture gives the latter ester (II), b.p. 155–62°/5 mm.; acid m.p. 239–40°; trianilide, m.p. 235°; anilide anil, m.p. 212°) on treatment with PBr_3 and undergoes cyclisation with sodium to form among other products ethyl 1: 1 : 2-trimethyl-Δ^2-*cyclo*pentene–5–one–3 : 4–dicarboxylate (III), b.p. 125–28°/3 mm. This ester is soluble in alkali, gives violet coloration with alcoholic $FeCl_3$, and does not react with semicarbazide or hydroxylamine, thus pointing to an enolic structure. On hydrolysis, it forms an unstable ketonic acid, which loses carbon dioxide on treatment with hydrochloric acid forming 1 : 1 : 2-trimethyl-Δ^2-*cyclo*pentene-5-one-3-carboxylic acid (IV), m.p. 186–87°; (oxime, m.p. 139–40°; semicarbazone, m.p. 225°). This on Clemmensen's reduction was expected to give *iso*lauronolic acid (V), but during the reaction, the ethylenic linkage was also reduced, forming mainly dihydro-*iso*lauronolic acid (b.p. 110–15°/20 mm.) from which *iso*-lauronolic acid has been obtained by methods already known.

$EtO_2C\cdot CH_2\cdot CH(CO_2Et)\cdot C(OH)CH_3\cdot CMe_2\cdot CO_2Et$
(I)

$EtO_2C\cdot CH_2\cdot C(CO_2Et):CMe\cdot CMe_2\cdot CO_2Et$
(II)


(III)

(IV)


(V)

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September 2, 1937.

[1] Walker, *J.C.S.*, 1893, **63**, 504.
[2] Perkin, *ibid.*, 1903, **83**, 854; Blanc, *C.r.*, 1896, **123**, 746.
[3] Koenigs and Hoerlin, *Ber.*, 1893, **26**, 813.
[4] Blanc, *C.r.*, 1906, **142**, 1085.

On the Use of "Current Bedding" in the Interpretation of Tectonics.

In recent years, the value of current bedding in determining the top and bottom of beds, and in fixing the chronological order of deposition, has been increasingly recognised, especially when dealing with highly disturbed and unfossiliferous strata. In curved current bedding, the top of the foreset beds is concave upward. The foreset beds meet the topset beds in an angle or in an abrupt curve, while they merge with the bottomset beds in a broader curve; that is, the current bedding is sharply truncated by overlying layers, while it comes in contact with the underlying layers by a tangential curve. This characteristic property of current bedding has recently been used with very great success by E. B. Bailey,[1] Th. Vogt,[2] and A. Allison,[3] in unravelling the complicated structure of the Highland Schists of Scotland.

During the month of April, while examining the southern portions of the Chitaldrug Schist Belt near Dodguni, the writer came across extensive outcrops of quartzite which are very beautifully current bedded and ripple marked. These quartzite beds were traced from near Dodguni for a distance of over four miles northwards, and the phenomenon of current bedding was seen throughout.

From the evidence of the directions of the dips, B. Jayaram[4] stated in 1919 that the main body of the schists in this area had been thrown into an anticlinal fold. The writer's examination of this region indicates that the rocks in this portion of the Schist Belt have been folded such that the beds generally dip away from the fold axis, and that the fold pitches in a north-north-west direction. From this, it does not, however, follow at once, that the fold is a true anticline. It may be—in Bailey's terminology an antiform. To decide which of these two types of folds is seen here, it is necessary to find out whether the succession of beds noticed in this area is normal or reversed. The presence of quartzites showing well-marked current bedding with truncated tops and asymptotic bases, naturally led the writer to use this technique for determining the order of succession of beds. Accordingly, detailed observations were made at a number of points, and the direction in which the beds presented a younger aspect, was carefully recorded (*vide* Map).

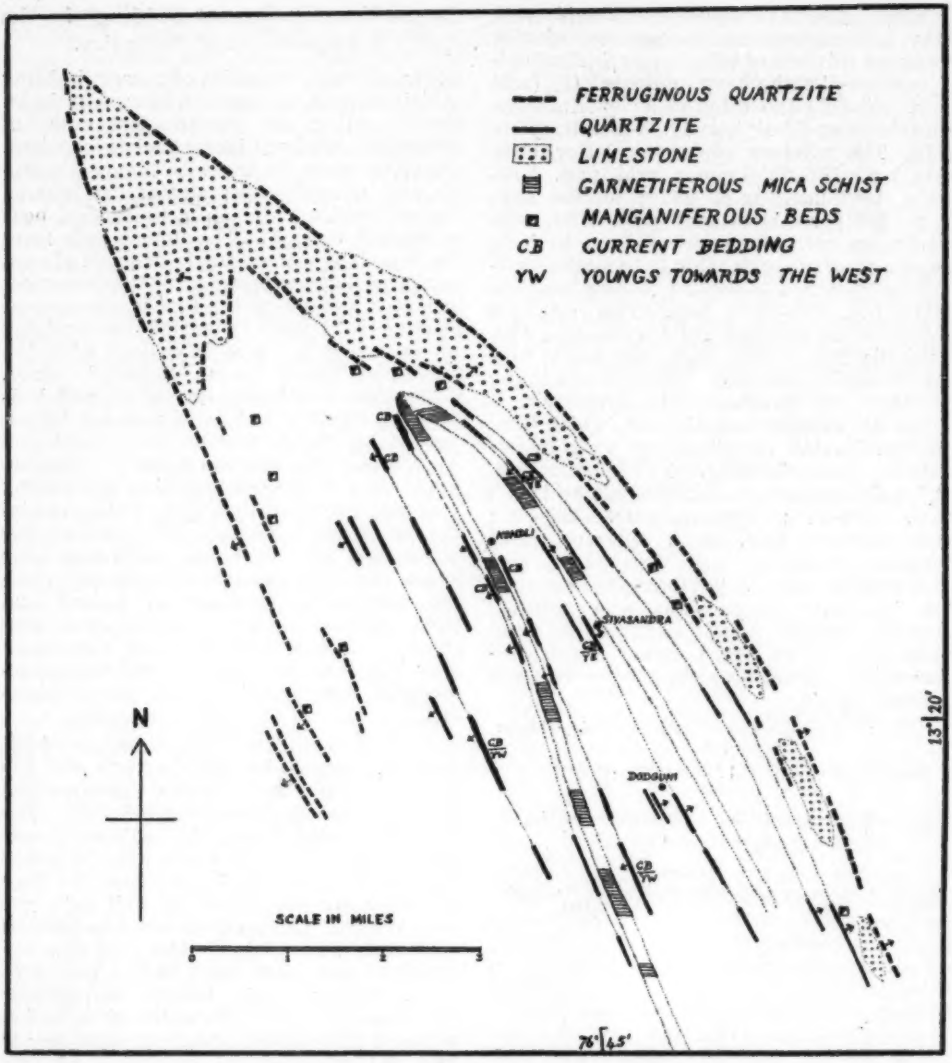


Fig. 1.

Sketch map showing the chief occurrences of quartzites, ferruginous quartzites, limestones and gametiferous mica schist. Outcrops of basic igneous rocks are omitted. Arrows indicate directions of dip.

Current bedding is very clearly seen in the quartzite beds occurring in the western limb of the fold. The quartzites sometimes dip very steeply, but without any exception, the evidence of current bedding indicates that the beds in this limb "young"[5] westwards. A very good starting point for this kind of investigation is on the quartzite outcrops about six furlongs south-south-east of Dodguni, alongside the road going from Dodguni to Nerlakere. Here, the strike of the quartzites is 10° west of north with a westerly dip, and the beds are seen to youngs westwards. Another convincing spot is on the forest demarcation line just north of the main road between Tumkur and Tiptur,

Fig. 2.

Current bedded quartzite, right way up; about six furlongs south-south-east of Dodguni.

near the 5th furlong of the 70th mile. The quartzite strikes 10° west of north with a dip of 45° westwards, and here again, the bed youngs westwards.

Similar observations made in the eastern limb of the fold, as for instance, near Sivasandra, show that the rocks uniformly dip east and young eastwards, the only exception to this being noticed at a spot about a mile north-east of Kondli. There is apparently a small inversion here, the exact significance of which is under investigation.

Thus, as the result of the use of this technique and in view of the consistent results obtained, two facts are clearly brought out in the interpretation of the tectonics of this area: (i) that the beds are normal and not reversed (with the one exception noticed near Kondli), and (ii) that the nature of the fold here is a true anticline and not an antiform.

CHARLES S. PICHAMUTHU.

Central College,
Bangalore,
August 9, 1937.

[1] *Geol. Mag.*, 1930, **67**, 77–81; *Quart. Journ. Geol. Soc.*, 1934, **90**, 462–525.
[2] *Geol. Mag.*, 1930, **67**, 68–73.
[3] *Quart. Journ. Geol. Soc.*, 1933, **89**, 125–44.
[4] *Rec. Mys. Geol. Dept.*, 1919, **17**, 82.
[5] E. B. Bailey, *Quart. Journ. Geol. Soc.*, 1934, **90**, 469.

Alternaria Species on Chilli in India.

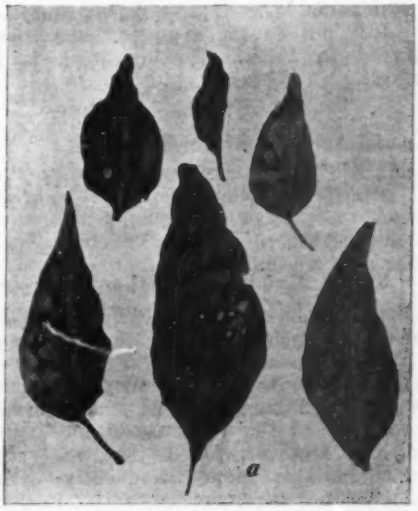
SPOTS of greyish or greyish-brown colour, oval, roundish or irregular in appearance with concentric rings were found on leaves, twigs and fruits of chilli plants in the vegetable gardens in the neighbourhood of Delhi.

The disease was rather severe and the damage done appeared to be considerable. The twigs dried up and the fruits became useless from the market point of view and failed to mature.

In the text-figures, *a* illustrates the spots found on leaves of chilli and *b* and *c* show healthy and infected fruits respectively.

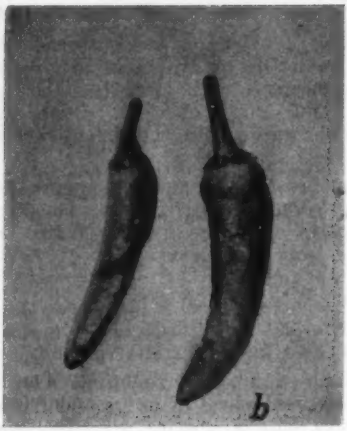
A large number of isolations were made from diseased parts of twigs and fruits and in every case a species of *Alternaria* was obtained. All isolations proved identical in their morphological characters.

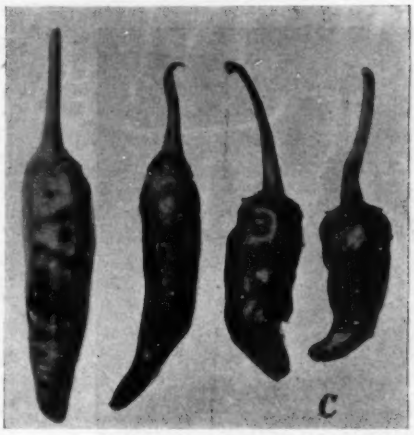
A large number of inoculations were made on chilli fruits to prove the pathogenicity of the fungus. The fungus infected wounded fruits within twenty-four hours and unwounded fruits in about forty-eight hours.



The spots that were formed agreed in appearance with those that occurred in nature on the fruit.

The morphology of the fungus was studied both on its natural host and in culture. Conidiophores are unbranched, septate and greyish-brown in colour and





are borne in clusters of 2–8. They are 25–70 μ in length and 4–6 μ in breadth. The conidia are greyish-brown to dark brown and borne in chains upto twenty, obclavate with a small beak in the majority of cases. They are 14–50 × 7–19 μ and with 3–5 cross septa.

Alternaria sp. has been recorded on chilli in Roumania, U.S.A. and other countries but does not so far seem to have been observed in India. Further work is in progress.

K. M. DUTT.

Imperial Agricultural Research Institute, New Delhi,
September 4, 1937.

The Antipodals of Verbenaceae.

THE antipodals in the family Verbenaceæ are described by Schnarf[1] in his review of the embryology of the angiosperms to be three in number, small and degenarating early. Junell[2] in his recent studies of the morphology of the Verbenaceæ and Labiateæ has, however, noted that the antipodals in this family frequently divide and their number increases, as for instance in *Petrea volubilis*.

The writer has seen this in one more member of the family. In *Clerodendron Phlomidis* Linn., there are at first only three antipodals (Fig. 1), but these very soon undergo repeated divisions and their number increases up to twenty or even a little more. These antipodals are small in size and so

Clerodendron Phlomidis.

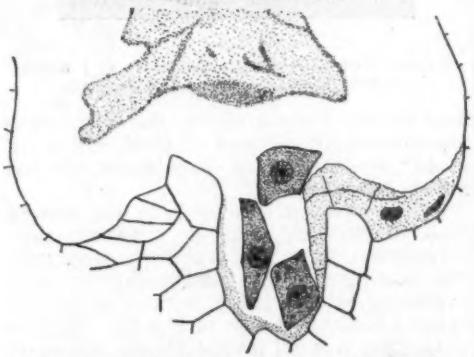


Fig. 1. Chalazal end of the embryosac showing three antipodals. × 650.

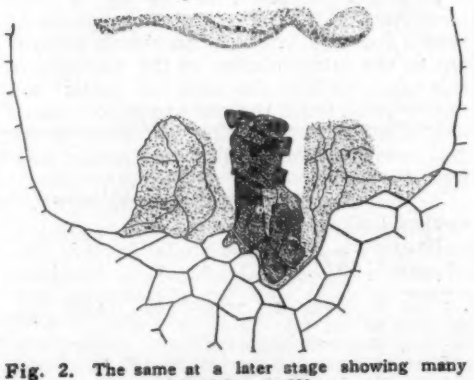


Fig. 2. The same at a later stage showing many antipodals. × 650.

are their nuclei, but they are very rich in protoplasm (Fig. 2). Only rarely an antipodal is found to be vacuolated.

The details of the embryology of this and some other plants of the same family shall be published elsewhere.

K. C. Misra.

Department of Botany,
Benares Hindu University,
September 4, 1937.

[1] Schnarf, K., *Vergleichende Embryology der Angiospermen*, Berlin, 1931.
[2] Junell, S., "*Symb. Bot. Upsalienses*, 1934, 1, No. 4.

The Occurrence of *Cystopus ipomoeae-panduratae* (Schw.) Swingle on *Ipomoea pestigridis* Linn.

Cystopus ipomœæ-panduratæ is reported on various species of *Ipomœa*, *viz.*, *I. eriocarpa*, *I. reniformis*, *I. hederacea*, *I. Batatas*, etc., in India and elsewhere also. So far as the writer is aware its occurrence on *Ipomœa pestigridis* is not reported from India.

The infected leaves of *Ipomœa pestigridis* were first collected at Utratia, District Lucknow, in September 1936. The disease manifests itself as white spots on the leaf (Fig. 1) which brings about distorted deformity of the leaf resulting prematurely in death. In older spots, the tissue crumbles down and holes are formed. The diseased plants with large ellipsoidal spots of dull white colour were also collected in Badashahbagh in October 1936, where the infection was visible on the petiole and stem also.

Fig. 1.
Leaf of *Ipomoea pestigridis* Linn., showing infection as white pustules. × natural size.

In spite of best efforts, only conidial stages were found, which are generally laid down in a thick sorus under the epidermis of the host and the conidial spores are liberated by its rupture. The spores are broad, ellipsoidal, smooth and are 16 to 21 μ long and 12 to 16 μ broad. The identification of the parasite was confirmed by the Imperial Mycologist, Delhi, to whom my thanks are due.

This species of *Ipomœa*, a diffuse, twining, pubescent herb, is very common in the United Provinces during the rainy season almost in all fields.

An effort to germinate the spores in culture was unsuccessful.

G. S. Verma.

Botanical Laboratory,
The University, Lucknow,
September 3, 1937.

Petalody in Cotton.

Ramanatha Ayyar and Sankaran[1] have described the appearance and genetic behaviour of a petaloid mutant from Karunganni cotton. They record complete dominance of normal in F_1 and segregation into 3 normal : 1 petaloid in F_2.

Seed of heterozygotes was kindly supplied to us by Mr. Ramanatha Ayyar, and grown at Indore. It was observed that the fertile plants in segregating families were of two kinds, those bearing normal flowers with never more than one or two petaloid stamens and those bearing flowers showing slight petalody in a quarter to half the stamens. Karunganni strains are so late in fruiting at Indore that very little seed was obtained. Nine F_2 plants with normal flowers (only an occasional stamen petaloid) gave 24 normal plants in F_3. Five F_3 plants with a quarter to half their stamens slightly petaloid gave 3 normal : 6 intermediate : 4 petaloid plants in F_3. Dominance of normal is therefore

incomplete, and heterozygotes can be distinguished with considerable accuracy.

J. B. HUTCHINSON.
R. L. M. GHOSE.
Empire Cotton Growing Corporation,
Cotton Research Station, Trinidad.
July 22, 1937.

[1] Ramanatha Ayyar, V., and Sankaran, R., *Ind. Jour. Agric. Sci.*, 1934, **6**, 938.

SUBSEQUENT examination of this character on a larger number of crosses has led us also to similar conclusions. Mention has been made of this feature in my report of the Cotton Breeding Station for 1936–37 (in press). A fuller article on this subject is under preparation.

V. RAMANATHA AYYAR.
Agricultural Research Institute,
Coimbatore,
September 6, 1937.

On the 'Fish-Gazer', *Ichthyscopus inermis* (Cuv. and Val.) Swainson[1] from the Madras Coast.

RECENTLY a fine specimen of *Ichthyscopus inermis*, a close relative of the 'Star-gazers', was obtained from the neighbourhood of the Madras Harbour. As the fish is not quite common, opportunity was taken to have the inside examined, and it was found to exhibit several interesting features.

The fish is very tenaceous of life and was alive two hours subsequent to being taken out of water. It measures 40 cm. in length from the snout to the tip of the tail and 33 cm. in girth at the base of the pelvic fin. It is beautifully coloured, the characteristic canary-yellow shades and the buffy-brown pattern being very well marked. The canary-yellow, however, has disappeared almost completely after preservation.

The highly muscular, sac-like stomach contained two recently swallowed specimens of *Sillago sihama*, measuring 10·2 cm. and 10 cm. respectively in length, besides partly digested remnants of smaller Teleosteans, egg capsules of Gastropods and some mud. The gastric epithelium exhibits folds all round, unlike the case in an allied form, *viz.*, *Uranoscopus cirrhosus*, reported from New Zealand, where the inferior wall is devoid of folds.[2] The pylorus is rather anterior with nine stout digitiform pyloric cæca. Possibly their number is variable in the species; for, Cuvier and Valenciennes mention only eight in a specimen from the Coromandal Coast.[3] Gunther also mentions only eight for the species.[3] The cæca are arranged seven in a semicircle, around the dorsal aspect of the duodenum and the remaining two in a short parallel line beside the posterior limb of the semicircle, towards the mesial side. They are sub-equal, their average length being 2·9 cm. The diameter of each cæcum approximates to that of the duodenum. The liver is horse-shoe shaped with the median part resting saddle-like on the œsophagus and the two limbs directed backwards over the sides of the stomach. The enormous gall bladder, filled with pale transparent bile recalls Aristotle's statement that Uranoscopus (called by him as Callionymus), has a gall bladder larger in proportion than that of any other fish. It is of interest to note that ancient physicians have ascribed to this bile the properties of healing wounds, consuming superfluous growths in the eyes and thus curing defective vision.

The ovaries are remarkable. They are of the cystovarian type and are placed in the hind region of the visceral cavity, where they form two closed oval sacs, uniting behind to be continued as a very short common oviduct which opens close behind the anus. They are slung to the dorsal wall of the body cavity by a tough mesovarium. The thick muscular wall of the ovary measures 1·4 mm. on an average in thickness and is composed of clearly defined circular and longitudinal bundles. The outermost layer is a tough fibrous one, made of intercrossing bundles forming a close feltwork which is closely invested by a thin sheet of connective tissue. Internal to this feltwork is a layer composed of slender longitudinal bundles intermixed with oblique and circular fibres. Next comes a thick layer consisting of well-developed fascicles disposed in a circular direction. Oblique and radiating prolongations of this layer into the stroma within, serve as axial supports for the ova-bearing folds. Such a remarkably thick, complex ovarian wall is rare among Teleosts. Usually it appears only as a comparatively thin sheet covering the ova. The ovary was seen to exhibit vigorous movements of contraction, which persisted for some time even after

the organ was severed from the body. The posterior position of the ovaries, taken in conjunction with the highly muscular walls which show considerable powers of contraction, makes one regard these movements as having a respiratory function, ensuring a constant change of water to the numerous ova inside. The presence of several Nematode parasites, discovered within the ovary, again shows the possibility of free access into the ovarian chamber from the outside.

When the ovaries were cut open in normal saline, numerous eggs were seen in what looked like a reddish yellow transparent mucous mass, which was sticky, hardening

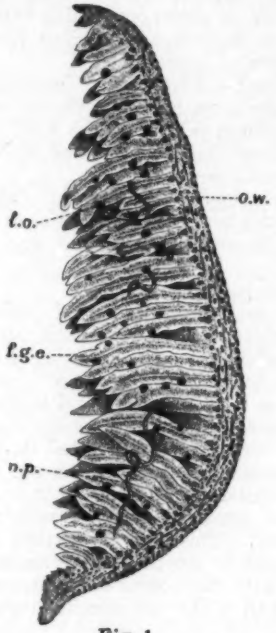


Fig. 1.

A longitudinal section through a portion of the left ovary. *f.g.e.*, fold of germinal epithelium; *l.o.*, large-sized ova; *n.p.*, Nematode parasite; *o.w.*, ovarian wall. Bouin, × 4.

into adhesive threads when pulled out between the fingers. Closer examination revealed that the mucous mass consisted of transparent closely packed finger-shaped 'villi' containing eggs. Upon fixing part of the organ in Bouin, the 'villi' contracted and took up the appearance of flattened transversely disposed plate-like folds of varying heights starting from the inside of the ovary and almost completely filling it (Fig. 1). Eggs in all stages of growth ranging from the earliest stage up to the most mature condition and showing different stages of yolk formation are seen in the stroma bounded externally by the thin germinal epithelium (Fig. 2).

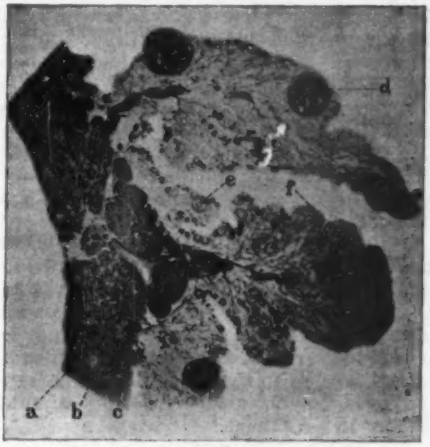


Fig. 2.

A photomicrograph of a section of the ovary showing two ovarian folds. *a—c*, wall of the ovary; *a*, outermost layer of intercrossing bundles; *b*, middle layer of mixed bundles; *c*, innermost layer of circular bundles; *d*, large-sized ova; *e*, medium sized ova; *f*, small-sized ova. Bouin, × 16.

My thanks are due to Professor R. Gopala Aiyar for his kind encouragement and criticism.

T. J. Job.

Zoological Research Laboratory,
University of Madras,
August 27, 1937.

[1] Syn., *Ichthyscopus lebeck* (Jordan and Snyder, 1902), *Uranoscopus lebeck* (Schneider, 1801).

[2] Cuv. and Val., *Hist. Nat. Des Poissons*, Tom. III, (Paris, 1829).

[3] Gunther, *Catalogue of Fishes in the British Museum*, London, 1860, 2.

Sexual Organs of *Pythium arrhenomanes* Drechsler on Artificial Substrata.

It is a well-known fact that sexual organs are readily formed in most species of *Pythium* on artificial media, but in some species they appear only rarely especially on ordinary

culture media in common use in most laboratories; *Pythium arrhenomanes* Drechsler is one of them. In this species it is possible to stimulate the production of sexual organs and oospores by using special media.

Johann[1] grew this fungus on grated carrot-agar. In each tube the grated carrot was placed up to a depth of about 2·5 cm. and then about 10 c.c. of melted water agar (7 gm. to 1 litre of water) was added. The tubes were then plugged with cotton and autoclaved. Sexual organs and oospores were abundant on this medium. He found that shreded fresh string beans were as satisfactory as carrot.

In the Drechsler's description of this species quoted by Matthews[2] (p. 57) it is mentioned that sexual organs and all oospores were obtained on carrot-cornmeal-agar.

The medium used by Rands and Dopp[3] consists of the humus extract (so-called humic acid) added to cornmeal-agar, which is tubed and then the grated carrot is added to each tube before autoclaving. Transfers to this have resulted in the development of normal oospores by 95% of the isolates. Synthetic humic acids did not stimulate oospore development.

The present writer cultivated this species in tubes (15 cm. × 1·5 cm.) and petri-dishes (8·5 cm. × 2 cm.) containing carrot extract agar. Carrots were cut into very small pieces and 100 gm. of these were kept in 1000 c.c. of distilled water in a plugged flask inside the steam-sterilizer for an hour. The extract was then filtered through a muslin and was made up to 1000 c.c. To this 1% or 2% Turner's Agar was added.

For the sake of comparison maize-meal-agar (Indian maize-meal 3% + agar 2%) and Johann's grated carrot-agar were also tried.

Pythium arrhenomanes Drechsler was obtained from the Centraalbureau Voor Schimmelcultures, Baarn (Holland) and *Pythium graminicolum* Subramaniam from Dr. B. L. Chona, Ph.D. (London), Sugarcane Mycologist, New Delhi, to whom I wish to express my thanks. The latter species was used for a comparative study. The fungi were cultivated at 15°, 20°, 25°, 30° and 35° C.

CULTURES IN TUBES.

1. *Carrot extract agar* 2%:—

(*a*) *Sexual organs.*—At 15° C. a large number of sexual organs were formed in *P. arrhenomanes* while only a few were developed in *P. graminicolum*. At 30° C. they were not formed in any species. 25° C. was the optimum temperature for the development of sexual organs of *P. graminicolum*.

(*b*) *Growth.*—At 35° C. there was some growth of *P. graminicolum* while *P. arrhenomanes* did not grow.

2. *Johann's medium*:—

When 2% agar was added to this medium sexual organs were more abundant.

CULTURES IN PETRI DISHES.

1. *Carrot extract agar* 2%:—

(*a*) *Sexual organs.*—They were not formed in any species when the medium was in thin layers (about ⅓ of the capacity of the petri-dish). In petri-dishes containing more medium (⅔ or ¾ of their capacity) only a few sexual organs were developed in each case, while in tubes containing this medium they were formed in sufficient numbers as already stated above.

(*b*) *Growth.*—There was luxuriant aerial and woolly growth in petri-dishes containing more medium while it was thin and non-aerial when the medium was in thin layers.

2. *Johann's medium*:

In each species the sexual organs were developed when 1% agar was used but their number decreased when agar was increased to 2%.

3. *Maize-meal-agar*:

In *P. arrhenomanes* sexual organs and a few oospores were developed but usually they underwent degeneration as already reported by Drechsler[4] (p. 682). In *P. graminicolum* sexual organs and more oospores were formed and their degeneration was less.

It has recently been asserted that *P. arrhenomanes* and *P. graminicolum* are synonyms but Drechsler[4] has clearly shown that they represent separate species rather than a single one. This fact is further confirmed on account of temperature relations in connection with the formation of sexual organs and growth of the two species studied by the writer.

A detailed investigation of the species is in hand.

R. K. SAKSENA.

Botany Department,
University of Allahabad,
August 25, 1937.

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[1] Johann, H., *Phytopath.*, 1928, **18**, 710.

[2] Matthews, V. D., *Studies on the genus* Pythium, 1931.

[3] Rands, R. D., and Dopp, *Phytopath.*, 1933, **23**, 745.

[4] Drechsler, C., *Phytopath.*, 1936, **26**, 676.

The Systematic Position of *Oidiopsis taurica* (Lév.) Salmon.

In a review of the recent phytopathological literature in India, Mundkur[1] has called in question the systematic position of *Oidiopsis taurica* var. *macrospora*, which has recently been described by us[2] as a new variety. In view of his remarks, a clear statement of the present systematic position of the species *Oidiopsis taurica* (Lév). Salmon seems necessary.

In 1902 Scalia first described the genus *Oidiopsis* for endophytic powdery mildews and referred to it his new species *Oidiopsis sicula* occurring on *Asclepias curassavica* in Sicily. This fungus was considered by Salmon[3] to belong to *Erysiphe taurica* Lév. as representing the conidial phase of the latter species. Salmon,[3] however, was not satisfied with the systematic position of *Erysiphe taurica* which he considered to be 'distinct generically from the other members of the Erysiphaceæ in its completely endophytic habit during the early part of its conidial stage, and in the production of usually branched conidiophores sent up through the stomata'. Confronted with this difficulty Salmon concluded that the best way to meet the situation was to establish a genus for the reception of *Erysiphe taurica* Lév. For this purpose he adopted Scalia's genus *Oidiopsis* and extended its characters to represent not only the conidial phase of the imperfect genus *Oidiopsis* of Scalia but also the perithecial stage of *Erysiphe taurica*. In this emended genus Salmon included the only species *Oidiopsis taurica* (Lév.).

Arnaud[4] in 1921 considered that Salmon should not have adopted the name of the imperfect form for the perfect one and proposed a new genus *Leveillula* and renamed *Oidiopsis taurica* as *Leveillula taurica* (Lév.). Some mycologists have followed Arnaud, whilst others such as Butler and Bisby[5] still prefer to use *Oidiopsis* after Salmon.

Judged in the light of this discussion, Mundkur's remark that 'not only the species *O. taurica* but even the genus *Oidiopsis* no longer exists' is rather misleading. He further adds to the confusion when he refers to *Oidiopsis* as the 'imperfect genus' as distinguished from the 'perithecial-stage genus *Leveillula*', although a reference to page 112 of our paper[2] will make it clear that the reconstructed ('perfect') genus *Oidiopsis* of Salmon is meant. It is likely, however, that in course of time the generic name *Leveillula* may be generally used as has been done by Gäumann and Dodge[6] and more recently by Bessey[7] and by Blumer,[8] in which case the fungus *Oidiopsis taurica* var. *macrospora* will bear the name *Leveillula taurica* (Lév.) Arnaud var. *macrospora* Uppal, Kamat and Patel. In the meantime, the binomial *Oidiopsis taurica* (Lév.) Salmon is quite in order.

Mr. S. F. Ashby, Director, Imperial Mycological Institute, London, to whom a reference was made in the matter, agrees with the above opinion.

B. N. Uppal.
M. N. Kamat.
M. K. Patel.

College of Agriculture,
Poona,
July 19, 1937.

[1] Mundkur, B. B., *Biochem. and Allied Res. in India*, (*Soc. Biol. Chem., India*), **1936, 7,** 120.

[2] Uppal, B. N., Kamat, M. N., and Patel, M. K., *Ind. Jour. Agric. Sci.*, **1936, 6,** 110.

[3] Salmon, E. S., *Ann. Bot.*, **1900, 20,** 187.

[4] Arnaud, G., *Ann. des Épiphyties*, **1921, 7, 1.**

[5] Butler, E. J., and Bisby, G. R., *The Fungi of India*, Calcutta, **1931.**

[6] Gäumann, E. A., and Dodge, C. W., *Comparative Morphology of Fungi*, New York and London, **1928.**

[7] Bessey, E. A., *A Text-Book of Mycology*, Philadelphia, **1935.**

[8] Blumer, S., *Die Erysiphaceen Mitteleuropas*, Zurich, **1933.**

Swarming of an Indian Cecidomyid Midge (Diptera).

Swarming in Cecidomyids is not a common phenomenon and as far as available literature indicates, it has so far never been observed in India. Kirby cited by Barnes,[1] (1927) observed the European wheat midge *Contarinia tritici* (Kirby) in large numbers round the wheat heads in the evening which disappeared at 7 P.M., but about an hour afterwards, the midges appeared again in swarms, laid eggs, and totally disappeared by 9 P.M. Wallengren[2] (1936) reported that appearance in large numbers of *Contarinia tritici* and *Sitodiplosis mosellana* Gehin was cyclic and stated that the period of greatest abundance in the former was about six days.

On Sunday, the 28th February 1937 at 10 A.M., the writer observed several swarms of a Cecidomyid in the vicinity of the Entomological Laboratory, Imperial Agricultural Research Institute, New Delhi. There were thousands of small black midges in each swarm which was therefore fairly thick and cast a shadow on the ground. The swarms disappeared at about 4 P.M. The swarms continued to appear every day in the laboratory compound upto about the 7th of March in the morning and disappeared in the afternoon. Evidently, the species swarmed when it was bright sunshine. The midges showed swift circling and backward and forward movements, and therefore from a distance it appeared as if the swarms were almost stationary. Sometimes quite a large number of midges of a swarm would alight on plants or on the ground and after a short time would again join the swarm. But as a rule the swarms were not seen settling on either cultivated or wild plants or elsewhere near or away from the place at which they appeared.

A large number of specimens were collected and sent to a specialist for exact identification. They have been declared to belong to a species new to science, which is being described as such (*Micromyia indica* sp. nov.) elsewhere.

H. L. BHATIA.

Laboratory of the Imperial
Entomologist, New Delhi,
August 3, 1937.

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[1] *J. South-Eastern Agric. Coll. Wye*, **24**, 80.

[2] *Lunds Univ. Arsskr.*, **30**, 70. Abstract in *Rev. Appl. ent.* ser. A. **24**, 348.

On Two Hyperparasites of Lac.

THE only way of indicating the importance of parasites among themselves is by giving a table of statistical records of such insect-findings. As far as I am aware, a quantitative analysis of frequency among parasites and hyperparasites of lac has not been studied. But I have found even qualitative or wide differences in the degree of association between lac insect and its associated parasites and hyperparasites not only according to the season and locality but also according to the species of lac insect.

The observations in Bangalore indicated that *Holcocera pulverea* is not predacious on *Lakshadia communis*. It is, however, very frequently found attacking the other lac insect, *Lakshadia mysorensis*, while, at the same time, *Holcocera* caterpillars are perhaps more parasitised in association with the Mysore lac insect than with any other of its kind. The species *Lakshadia mysorensis* therefore gives an excellent insight into the association of *Holcocera* with its own parasites. *Apanteles lachardiæ* came in the first rank, while *Apanteles fakhrulhajiæ* was its next most effective parasite. This was found also to hold true in studying the insect fauna associated with *Lakshadia nagoliensis* in Sohagpur, Central Provinces.

In Bangalore, *Chelonus cycloporus*, Franz,[1] is rare or unknown, but occupies the third place in descending order of importance in Sohagpur. It is surprising this insect has not been recorded by any other worker on lac. Fig. 1 shows the female *Chelonus cycloporus* and the illustration may now help others to confirm my findings.

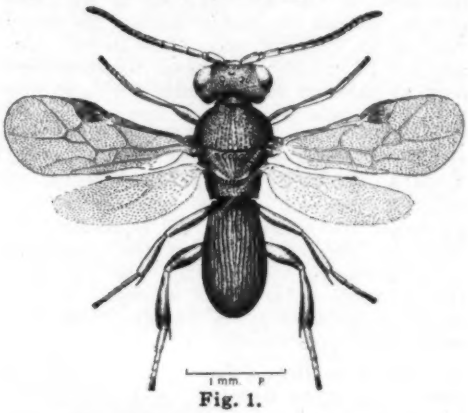

Fig. 1.

Female *Chelonus cycloporus*, Franz.

Young[2] records rearing an Ichneumon from lac and possibly the same species was illustrated by Stebbing,[3] as I have already tried to explain.[4] Stebbing illustrated a male Ichneumon, while Fig. 2 here records the female of the same insect. It is strange Stebbing's finding has not attracted any comment from any other writer on lac. I identified my specimen as *Pristomerus* and as I found no other insect of this genus it must be identical with *Pristomerus testaceicollis*, Cam., which, according to Glover,[5]

(*Continued on page 119*)

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